September 11, 1953

Dear Andy:

Our affairs are beginning to become re-ordered following our trip to San Francisco and back, and moving into a new house, and the convention of American Biologists on campus. We envy your attendance at the meetings at Rome— you can be sure that the sole obstacle to our own was financial. I am just getting back to work, though I think I shall be spending more time this year on E. coli cytology than on Salmonella serology. There are some loose ends on the latter, however, and at any rate I expect the program to be continued in full force in Bernstein's hands. His arrival is expected tomorrow. Mrs. Bifford was obliged to cancel his reservation in deference to some University rules for student housing. However, this is probably all to the better as BorisRotman has just rented a large apartment and hopes to interest Bernstein in sharing it with him.

Thank you ever sommuch for your letters and materials received during the summer. I have just finished a brief look at the k phage, and find the stock preparation itself to be a highly competent transducing phage with either SW-666 or (Kauffmann's O-248, monophasic para'B) or O-901 as indicators. If nothing else, this has been the first means of using S. typhi as a transductional source, and has given for example:

S. typhi $\overset{k}{\longrightarrow}$ x SW-666: IV V XII d:—

In my hands also, your preparation of k seems entirely free of bacteria.

The para'B typing phage BAOR seems also to be competent in transduction, but with a host range rather more limited than PLT22. A few plaques have been noticed on some kunzendorf strains, and have since been seen also with PLT-22. I am not sure whether these might not represent contaminating phages. At any rate, I have not succeeded in demonstrating transductions to any group C strains, nor in building up either of these phages to an appreciable titre on them. The O phages, 1,2, and 3 have also given disappointing results having been tested, in particular against a strain of S. typhi O-901 lysogenized for k (and, as you say, thereby immune to the O phage) as indicator. I hesitate to insist on any such negative results, however, and further tests are in order.

As you know, Dr. Felix has refused my casual request for the Vi typing phages. I can fully appreciate this policy; at any rate it is of no great immediate concern. In due course, I will be happy to have them, when available.

I was interested to see the protocols on the host range of the B phages you kindly sent on July 16. I was interested in the frequency of heterologous reactions, especially in group C. I wonder if it would be too much of an imposition to ask either for the specific strains of cholerae suis (1348);newport,PortoRico; and fayed, or in addition for the additional information as to which particular phage in the Pool 2 they are reacting with. I suspect that these protocols reflect only the occasional positive reactions you encountered, and not necessarily all the cultures and types tested. If you should have occasion to test the reactions of divers Salmonellas with k, I should also be interested to hear of them: again, the reactions with bacterial strains outside the A-B-D groups would be of most immediate concern. Finally, k has been working so well that I wonder whether you could be imposed on to send some of the other determinate phages (especially if serologically distinct) at

some future time convenient to yourself.

We were very pleased last weekend to entertain Bill Hayes. He certainly is the personable chap you described him to be, and we were happy to make this personal contact [especially as there have been some entirely gratuitous attempts to inject non-existent personal issues in this scientific controversy]. At least at the instant, he agreed that the findings on our diploids, e.g. that some of these may show one segment eliminated from the F+ parent, an adjacent segment from the F-, are essentially fatal to the notion of a pre-zygotic defect in the gametes as he and Watson had previously propesed. I am pleased to note that he plans to continue efforts to isolate the F agent which, if successful, may allow a decisive test of his vector hypothesis. Barring this, the differences between our theories are largely verbal. On our side, we have had some encouragement in cytological examination of Hfr x F- [possibly I showed you some of the slides showing what might conceivably be conjugation figures], and I am planning to spend some time to test the possible role of these figures in the mating process.

Please give our best to Clive— I hope to send off a letter to him (and to Bruce) in a couple of weeks, after things have settled down.

Sincerely,

Joshua Lederberg